

LETTERS TO THE EDITOR.

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A New Oceanographical Expedition.

IN NATURE of November 18 (p. 71) there is a notice of a new oceanographical expedition, to be undertaken by the Norwegians in their surveying vessel *Michael Sars*, on the suggestion of Sir John Murray, and mainly at his expense. It is very gratifying to meet with cooperation of this kind in the prosecution of deep-sea research, and the investigation of the portion of the North Atlantic contemplated in the programme cannot fail to furnish interesting and useful results.

In the account of the expedition I note the following passage:—"The application of methods of high precision to the determination of the temperature and salinity of sea-water has yielded results which have raised considerable doubt in the minds of some investigators as to the validity of the earlier observations made by the *Challenger* and other expeditions, and the cruise of the *Michael Sars* should not only afford much entirely new information, but provide a means of valuing the earlier work."

As chemist and physicist of the *Challenger* expedition, I feel that this is a reflection, not only on the name of *Challenger*, but also on myself. I was a professional chemist of recognised standing at the date when the expedition was planned, and it was to this fact that I owed my selection for the post nearly a year before the ship sailed. During the whole of this time I was occupied with the study of the work to be done and of the methods to be employed in doing it. Some of these were devised by myself, and none were approved before they had been thoroughly tested on land; nor were they finally accepted until they had passed the probation of the first three months at sea. The regular work of the expedition began with the sailing of the ship from Teneriffe on February 15, 1873. By this time the scheme of the routine work of my department had taken definite shape, and it suffered but little alteration during the cruise. All the actual work was done by myself, and no method was employed which I had not myself tested and found to give, in my hands, thoroughly trustworthy results.

I think it is due to me and to the readers of NATURE that the investigators, in whose minds doubts have been raised as to the validity of the *Challenger* observations, should state them, with the grounds on which they rest, and also indicate how they expect the cruise of the *Michael Sars* to provide a means of valuing the earlier work.

November 27.

J. Y. BUCHANAN.

Gametogenesis of the Sawfly *Nematus ribesii*. A Correction.

IN the *Quarterly Journal of Microscopical Science*, vol. II., 1907, p. 101, I described observations on the gametogenesis of *Nematus ribesii*, some of which subsequent work has shown to be erroneous. Since my statements have been quoted in several recent papers, I think it necessary to correct the mistakes as far as possible, although I have not yet reached a satisfactory solution of the phenomena. The errors arose partly through misinterpretation of the phenomena observed and partly through imperfect fixation, for I find that, unless the material is very accurately fixed, the chromosomes tend to adhere together and give the appearance of a smaller number than the true one. The same cause has led other observers to make similar mistakes.

Re-investigation of *Nematus* shows, in the first place, that there is only one division of the spermatocytes; the first division described in my paper is not a true mitosis, but is probably comparable with the abortive division observed in the spermatogenesis of the bee. I have not yet been able to determine the chromosome number with certainty. In the spermatogonia the number appears to be about sixteen, and that in spermatocyte mitoses about eight, but if eight is the true reduced number, the occurrence of sixteen in the spermatogonial mitoses of larvae derived from parthenogenetic eggs is unexplained. In the bee, and

as I find also in a Cynipid (to be published shortly); the spermatogonial number is the same as that of the spermatocytes.

I have not yet obtained fresh material for re-investigation of the maturation of the egg, but the results of my recent work on the spermatogenesis make it clear that my observations on the chromosomes in the polar divisions also require revision.

But the behaviour of the chromosomes in *Nematus ribesii* is so difficult to follow that it is possible that the true interpretation will be obtained only by the discovery of some nearly related species in which they are more clearly distinguishable.

LEONARD DONCASTER.

University of Birmingham, November 27.

Are the Senses ever Vicarious?

[PROF. MCKENDRICK has sent us the subjoined letter received by him, and his comments upon it.—ED. NATURE.]

My attention has just been directed to a letter which appeared in NATURE of March 11 (vol. lxxx., p. 38). It was signed by Prof. McKendrick, and dealt with the vexed question of the blind and their faculties.

I am a blind man, and have mixed with blind people of all ages for the past thirty years. You will grant that I ought to know something about the question you discuss in your letter.

Permit me to thank you for what you say about the popular notion that when a person loses his sight he is compensated by a gift of ability in one, if not all, his other faculties. The intelligent blind know how foolish this idea is, and constantly protest against it. The public, however, insist upon its accuracy, and calmly assume that the blind do not grasp the point at issue, or affirm that those who protest are unbelievers in the goodness of God. This assertion of compensation leads to all sorts of ridiculous notions, and has a very pernicious practical effect. The very people who assert the theory of compensation are among the number who shrink from providing facilities for the proper training and employment of the very gifted people they profess to look upon as the possessors of special talents. They impute to us the possession of all kinds of striking abilities, yet they decline to allow the specially talented to do what would earn or help to earn a livelihood. We are credited with marvellous powers in music, basket-making, &c., and yet when we assert our claim to live the ordinary life of the citizen these people are shocked at our audacity.

Now, the overthrow of the theory that we are specially compensated for the loss of sight will destroy the false impressions regarding our wonderful memories and all the other fantastic notions, and the way will be opened for common-sense treatment of the training and employment of the blind. It is notorious among the blind themselves that numbers of them are not at all musical, and that mechanical ability is not a conspicuous feature. Many blind are very deficient in hearing, in smell, and in the sense of touch itself. My own experience has compelled me to take heed of the varying degrees of what I shall call, for want of a better name, ear-power. The same variety exists in touch-power and memory-power. I should like to refer to these as well as many other interesting phenomena, but I fear I must content myself with asking your kindly attention to a problem which has baffled me for more than twenty years. Why does the voice call up before me the upper part of the speaker's face, and enable me to form a picture of the expression of the speaker? The expression of the eyes is frequently as vividly before me as when I could see. When people are speaking to me, they are never on guard to control their countenance as they would be if conversing with a sighted person. I am thus enabled to get a picture of the play of their emotions which helps me to come to conclusions as to character, &c. The lower part of the face was only once made visible to me, so that I could feel sure about it. I know when a person smiles, frowns, when the face lights up with an intelligence or when apathy and want of perception cloud the countenance. Sometimes I can follow the line of the glance and can point out where it would strike. When listening to public speakers I like to sit at an angle to them, and not in front. Can you point to anything that will aid me to come to a sensible conclusion on this matter of the voice convey-

ing the picture of the upper part of the face, and thus help me to fathom a question which I am persuaded contains the key to many other problems as to the constitution of sound and the organ of sound?

GEORGE IRONS WALKER.

Westbury Street, Sunderland, October 28.

WITH reference to Mr. Walker's interesting letter, which bears out the opinion of Prof. Kunz and others that there is no special development of the other senses in those who have lost the sense of sight, I feel at a loss to give an adequate explanation of the curious experiences described by Mr. Walker. The only suggestion I would venture to make is that Mr. Walker may, by long and almost unconscious practice, have learned to associate certain tones of the voice, as regards quality of tone, with certain movements of the head that he supposes are made by the speaker at the time he utters the words. Tones of inquiry, surprise, reproof, affection, interest, have each a certain quality indicative of states of feeling (unless they are produced by mimicry), and the blind man may draw conclusions as to movement and state of feeling on the part of the speakers. He has then what Mr. Walker calls "a picture of the play of their emotions." I cannot explain why Mr. Walker has almost invariably a picture of the upper part of the face, nor why he prefers to sit at an angle to a public speaker instead of in front. His experience supports the view that the blind have not more acute sensory perceptions than those who see, but that they have cultivated the habit of close attention. This, in turn, stimulates their imagination, and gives them mental pictures of external things that are of no special importance to those who see.

JOHN G. MCKENDRICK.

Movements of the Red Spot Hollow on Jupiter.

TRANSIT estimates of the Red Spot Hollow on Jupiter, obtained between 1908 December 20 and 1909 June 12 inclusive, show that that object exhibited an average monthly increase in longitude of 1.03° . Its motion, however, was not constant, inasmuch as it remained practically stationary in longitude during the last three months (April to June) of the apparition. The rotation periods of the three selected points of the Hollow, namely, the two shoulders and the middle, work out as under:—

p. Shoulder.

Date	Long.	No. of transits	Elapsed rotations	Mean daily drift	Rotation period
1908, Dec. 20	358.5°	16	... 408	... -0.0376°	... 9 55 42'
1909, June 7	48°				

Middle.

Date	Long.	No. of transits	Elapsed rotations	Mean daily drift	Rotation period
1908, Dec. 20	13.6°	15	... 420	... -0.0274°	... 9 55 47'
1909, June 12	18.4°				

f. Shoulder.

Date	Long.	No. of transits	Elapsed rotations	Mean daily drift	Rotation period
1908, Dec. 20	31.1°	20	... 420	... -0.0344°	... 9 55 42'
1909, June 12	37.1°				

The mean rotation period of the Hollow, therefore, appears to have been, as nearly as possible, 9h. 55m. 42s., a period which is 1.4 seconds longer than that of the adopted value of System II.

At the commencement of the observations, in December, the middle of the Hollow crossed the central meridian about twenty-three minutes subsequent to the passage of the zero meridian, and half an hour at the close of the apparition in June. This lagging behind may be regarded as a distinctly normal movement on the part of the Red Spot.

When the planet was observed last month as it emerged from the sun's rays, the Hollow was found to have moved at an accelerated rate of velocity during the unobserved interval since June. From transits obtained on October 15, 25, and 30, the deduced mean longitude of the middle of the Hollow was then 16.4° . This shows a decrease of 2° when compared with the longitude for June. It is evident, therefore, that the motion of the object had latterly become quickened. Had the Hollow continued to drift at the same rate as was exhibited from December to June, it would have crossed the central meridian ten minutes later than was actually the case last month. Owing to this slight displacement in longitude, the rotation period from June to October was shorter than that for the previous six months, and works out at 9h. 55m. 40s.

Leeds, November 21.

SCRIVEN BOLTON.

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Secondary Kathode Rays.

IN a letter to NATURE of April 2, 1908 (vol. lxxvii., p. 509), I described some experiments of mine which showed that for the corpuscular rays produced in metals by Röntgen rays there was a lack of symmetry between those coming from the side of the metals on which the primary rays were incident and those coming from the side from which the primary rays emerged. The ionisation produced by the emergence secondary corpuscles was, in general, greater than that produced by the incidence corpuscles. This was in accordance with Prof. Bragg's results for the corpuscular rays produced by γ rays (NATURE, January 23, 1908, p. 270).

Since writing the above I have endeavoured to see if this lack of symmetry was dependent on the penetrating power of the primary Röntgen rays. Experiments were carried on only with gold and silver, and gave the following results. The average of four determinations with soft primary rays on silver showed the ionisation produced by the emergence to be 1.11 times as great as that produced by the incidence corpuscular secondary rays; eight determinations with hard primary rays gave an average ratio of 1.21. Five determinations with soft primary rays on gold gave the ratio of emergence to incidence ionisation as 1.03; nine determinations with hard primary rays gave a ratio of 1.09. The probable error of the mean in each case was ± 0.01 . It would seem, therefore, that there is a slight variation of the asymmetry with the hardness of the Röntgen rays, certainly in the case of silver, and very probably in the case of gold, the harder primary rays causing the ratio of the emergence to the incidence corpuscular rays to increase.

Though the hardness of the Röntgen rays could be varied, they were probably always very heterogeneous. I hope soon to repeat my experiments, using more homogeneous Röntgen rays, which have been recently made possible by the experiments of Prof. Barkla.

CHARLTON D. COOKSEY.

Sheffield Scientific School, Yale University, New Haven, Conn., November 17.

AN INTERNATIONAL MAP OF THE WORLD.

AN International Committee assembled in London on November 15 to consider the form in which it is desirable to prepare a uniform map of the world on the scale of $1/1,000,000$, or about sixteen miles to the inch.

The proceedings of this committee have aroused keen interest among geographers, and the results of its labours will be anxiously awaited. The meeting of this committee marks an epoch in map-making, and if its proposals are generally adopted, as no doubt they will be, there will be prepared a map of the whole world, uniform in design and execution, on a reasonably large scale.

Hitherto each country has, in the preparation of its maps, had in view solely its own requirements, and has made no effort to assimilate its maps to those of other countries, either in regard to scale, projection, method of representing hills, or in other points. Maps have been issued differing widely in these respects from those even of the adjoining countries.

The difficulty caused by this diversity of map design has long been felt, not only by those little versed in map reading, but by those who have constant occasion to work with maps.

It was not until 1891 that the first important step was taken towards obtaining a more uniform map of the world. In that year the International Geographical Congress at Bern raised the question, and the London Congress of 1895 passed a resolution recommending the scale of $1/1,000,000$, or about sixteen miles to the inch, as suitable for a map of the world. This resolution was communicated to the various Governments in the hope that this scale might be generally adopted.